March 9, 1951.

Dr. Tom Nelson,
Kerkhoff Laboratories of Biology,
California Institute of Technology,
Pasadena 4, California.

Dear Tom:

I am sorry to hear of the shadow on your prospects at Caltech for next year. Except for industrial or government openings, there haven't been many opportunities come to light. Are you interested in these? Universities generally have become very wary of taking on new staff personnel owing to the prospects of dropping enrollments, and this is particularly acute at state institutions. I suppose that applications are still being taken for some fellowships, notably another round of PHS. I wish it were possible to invite you to name this department in such an application, but I am afraid that space reallocations and remodelling are being delayed in a way that leaves us barely able to meet present commitments. However, if you should become interested in some such arrangement, especially for early 1952 or later, let me know and I'll keep you posted.

Max was here a couple of months ago and mentioned his qualifications to your kinetic work. He particularly mentioned a few points in your technique which I did not quite understand - viz. shaking suspensions in flasks with a layer of agar. I hope your paper (in Genetics in Press?) gives enough detail so that we can try to repeat some of the experiments. I still think that kinetics, by itself, is a waste of time unless used as a direct guide for more tangible experiments. Have you found any recombination inhibiters in supernatants of old broth cultures? I can't pretend to understand the s/ story: what's the difference between liquid and agar? You say that s/ x s- gives all s/. How about s- x s- under the same conditions? By the way, you could have saved some trouble with your K-12 s/ by using streptomycin-minimal and crossing with W-1177 (which Dulbecco has, I think). It's perfectly all right to give out the cultures you mentioned, but I'd like to keep track of who has them. Who has any use for W-1205? Also, I notice that Ryan "published" Y-9 as 679-680-pab (in MCB). To the best of my recollection, this does better with pab than meth. But it's a lousy mutant, and its only interest is that it was the first mutant to be isolated by limiting enrichment. I am interested to hear so much activity with bacteria at Caltech-is phage petering out?

Most of my own time now is spent on a routine picking out of new fertile strains (about a dozen from over three hundred screened), and in cleaning up (the data, not the interpretation) of heterozygote behavior. Esther is working on the segregation of lambda, and similar genetics. Norton Zinder has a couple of Salmonella mutant stocks which are perfectly stable, and give very high yields of prototrophs when mixed, but the unselected markers so far tested have not shown very many new combinations. Are you coming east for the SAB meeting in Chicago (ca May 30), or CSH (June 8-15)? Lots of jobs are picked up that way.